your journal on the need of establishing natural history museums in the principal towns of our country. The ideas set forth cannot fail to be reciprocated by a largely increasing number of students who, like myself, are suffering under the disadvantages of not having local museums for reference and in which to compare specimens and examine the various natural history objects which I wish to study. In addition to a museum, I think such buildings should contain lecture-rooms specially fitted up for scientific lectures, as the value of able discourses is frequently lost for want of clearness in illustration.

The professor cannot over-estimate the value of museums, as every lover of natural history cannot be a collector; but every one in full possession of his faculties can observe so far as he has the power of seeing, and if he cannot examine the wide field of nature for facts he will at least examine the proofs of them in the museums, if at hand.

A few personal observations may serve to show the difficulties under which the so-called working classes have to labour in the pursuit of knowledge.

Some years ago I began to study the works of Sir C. Lyell and other authors on geology, and while so engaged I many times travelled eighteen miles after a hard day's work to compare specimens in the old museum, St. Peter's Street, Manchester. I had tabular views of the characteristic British fossils at hand, but as perfect specimens only are figured, I experienced a doubt and uncertainty pretty nearly in everything I wanted to compare, while in the museum I could find the actual specimen sought after with which to correlate those of my own. The flash of satisfaction experienced by a collector on comparing his objects with those in a well-arranged museum is indeed very great, and there are few things more likely to stir him up to renewed efforts. But the interest of museums is not confined to the collectors of natural history objects; it extends to every man who reads and cares to master the objects about which he reads. In this way his knowledge of things becomes real and he expresses himself with confidence, and in many cases has decided while others are thinking. To show further the need of museums I may state a fact perhaps not generally known, that in one place in the north of England a large number of science students have formed themselves into an itinerant society moving from place to place to suit the convenience of the various members who reside apart. The meetings are generally held at a respectable inn on Sunday evenings, at which papers are read by the more ambitious members, and any interesting objects named, which some of the party never fail to bring up, and their habitat declared.

If the corporate bodies or the educational department of the State would only undertake to provide museums in the principal towns of our country I feel sure that the cry of continental superiority would soon vanish. At home we have the materials out of which the philosopher and the artisan can spin the fibre of future greatness by rightly directing the forces of nature, but the isolated fragments want collecting and receptacles providing in which to store them. Many lives like that of the Banff naturalist could be written if only known, and Prof. Dawkins could not have fixed on a centre of operation more favourable from which to begin than that of Oldham. Men more selfishly removed above praise, working for science for its own sake, he cannot find, and it is a pity that they have not a common repository in which to store their invaluable collections beyond their own full cabinets. I hope the professor's articles will be a means of calling attention to the desirability of establishing museums for the better diffusion of scientific knowledge.

I write from the point of view obtained by my own experience as a working man who has done his best to educate himself.

WM. WATTS

Corporation Waterworks, Oldham, June 16

Koenig's Tuning Forks

ON vient d'attaquer en Angleterre l'exactitude du diapason officiel français. Mr. Alexander J. Ellis ayant trouvé que les notes d'un tonomètre, composé de 65 anches d'harmonium et construit par Mr. Appunn, ne s'accordaient pas avec ce diapason, a cru devoir déclarer dans un mémoire publié par le *Journal of the Society of Arts* (25 Mai, 1877), et dans votre journal (31 Mai, 1877), que le La_3 normal français donnait non pas 870 vibrations simples, comme on l'avait cru jusqu'à présent, mais bien 878 vibrations simples.

Mr. Ellis ayant constaté de plus que les diapasons de ma construction s'accordaient parfaitement avec le La_3 français, n'a pas hésité à affirmer que tous ces diapasons, y compris ceux de mon grand tonomètre, qu'il n'a probablement jamais vus, et en tout cas jamais pu examiner, étaient nécessairement inexacts. N'ayant pas à ma disposition l'instrument dont s'est servi Mr. Ellis, j'avoue que je me serais trouvé assez embarrassé pour dire immédiatement, par où pèche cet instrument au point d'avoir donné entre les mains de Mr. Ellis des résultats si extraordinaires; heureusement je me suis rappelé une lettre de M. Helmholtz à Mr. Appunn et publiée par ce dernier lui-même dans une brochure sur les théories acoustiques de M. Helmholtz; cette lettre concerne justement un instrument de même nature du même constructeur et explique suffisamment les surprenantes découvertes de Mr. Ellis. "J'ai examiné à plusieurs reprises votre tonomètre," écrit M. Helmholtz à Mr. Appunn, "et je suis étonné de la constance de ses indications. Je n'aurais pas cru que les anches pussent donner des sons aussi constants que ceux que donne l'appareil, grace à votre méthode pour régler le vent. L'instrument varie un peu, il est vrai, avec la température, comme feraient aussi des diapasons; on ne peut donc s'en servir pour la détermination des nombres absolus de vibrations que lorsqu'on peut travailler dans une pièce qui n'est pas chauffée par un poêle. J'ai compté les battements à l'aide d'un chronomètre astronomique, et je crois que votre pendule à secondes a été légèrement inexact, car, si les nombres de battements s'accordent très bien entre eux, le nombre absolu en a été non pas de 240, mais de 237 à la minute. La température, qui était assez basse pendant mes expériences, a pu y être pour quelque chose, mais on peut éliminer cette influence en comptant jusqu'au bout les battements d'une tierce majeure, ce qui m'a pris un quart d'heure. J'ai trouvé ainsi pour mon diapason de Paris 435·01 vibrations, ce qui l'accorde à $\frac{1}{40,000}$ pris avec le nombre officiel de 435·00 vibrations."

Cette lettre prouve que le nombre entier des battements de l'octave du tonomètre essayé par M. Helmholtz était de $\frac{237}{60}\cdot 64 = 252\cdot 8$, et sa note fondamentale de 505·6 vibrations simples au lieu de 512 vibrations simples. En comparant cette note de 505·6 vibrations simples avec un diapason donnant réellement 512 vibrations simples, Mr. Ellis eût trouvé ce dernier de 6·4 vibrations simples plus aigu, et l'eût sans doute considéré comme donnant 518·4 vibrations simples. Or il a trouvé 516·7 seulement pour mes diapasons de 512 vibrations simples avec le tonomètre dont il s'est servi; on voit donc que la note fondamentale de ce dernier était déjà plus exacte que celle du tonomètre examiné par M. Helmholtz puisqu'elle donnait 507·3 vibrations simples mais qu'elle restait encore assez loin de la véritable valeur.

Le fait que M. Helmholtz a pu trouver le nombre de vibrations exact du diapason officiel français avec un instrument de cette nature (et même encore moins parfait que celui dont s'est servi Mr. Ellis), en déterminant d'abord la correction de cet instrument, montre à l'evidence que Mr. Ellis a négligé de déterminer la correction du sien; il s'est donc beaucoup trop hâté de déclarer que ces petits tonomètres à anches d'harmonium sont les plus parfaits et les plus exacts qui existent, et de contester si légèrement les résultats obtenus par les Lissajous, les Despretz, les Helmholtz, les Mayer, etc., etc. RUDOLPH KOENIG

Paris, le 5 Juin

Antiquity of Man

MR. SKERTCHLY is absolute that I am mistaken; to me it appears that he has missed the point of my letter, and misinterpreted my views. His important discoveries of flint implements in early glacial beds are, I think, strongly corroborative of the opinions I expressed in my paper on the "Drift of Devon and Cornwall" (*Quar. Journ. Geol. Soc.*, vol. xxii. p. 88), and in that on the "Geological Age of the Deposits containing Flint Implements at Hoxne" (*Quar. Journ. Science*, July, 1876); but I willingly admit that in the present stage of the inquiry Mr. James Geikie has as much right to claim that they support his theory, and I agree with the latter that it is premature to discuss the relation of man to the glacial period, before we have settled what was the succession of events that occurred at tha time.

Mr. Geikie contends that there were two or more glacial periods with inter-glacial warm or mild ones; I, that there was

only one glacial period and that the disappearance of palæolithic man from Northern Europe was principally due to the submersion of the greater part of the land beneath the water of an immense freshwater lake or sea, at or a little before the culmination of the ice age. If Mr. Geikie's views should be ultimately accepted, the term "inter-glacial" will be most appropriate; but should, as I hope and believe, mine be proved to be nearer the truth, I should prefer to use the term "pre-diluvial" instead of "pre-glacial," as heretofore, to express the age of palæolithic man.

THOMAS BELT

The Cedars, Ealing, June 22

WILL you kindly allow me to correct an apparent breach of official etiquette and act of discourtesy in my last week's letter? I should have said that only two geologists prominently interested in the question at issue had seen my evidence; for, of course, Mr. H. W. Bristow, F.R.S., Director of the Geological Survey of England and Wales, has been kept fully *en rapport* with my work, and has several times visited me at Brandon. I am anxious that no statement of mine should appear to slight so eminent a geologist and so considerate a friend.

Brandon SYDNEY B. J. SKERTCHLY

Colour-Sense in Birds—Blue and Yellow Crocuses

UNLESS your readers are quite tired of the subject, may I add a fact which will be subversive of a good deal that has been written about yellow crocuses and sparrows. I dislike yellow crocuses, and four seasons since planted some hundreds of blue and white in the garden underneath my windows. For two seasons they flowered in beautiful profusion. In 1876 the sparrows for the first time destroyed these flowers completely. I allowed the roots to remain for another year—viz., 1877—but they suffered the same usage, hardly a single flower being left uninjured. So complete was their destruction that I have had the roots dug up.

I regard the proceeding as an imitative one; blue and white crocuses, not being common in the vicinity, were new to the sparrows, and until one more experimental than the rest attacked them they were safe.

A similar result will occur with domestic pigeons; if reared exclusively with small grain, as wheat and barley, they will starve before eating beans. But where they are thus hungry, put a bean-eating pigeon amongst them, and the habit is immediately propagated.

I have seen fowls refuse maize at first, but on seeing others eat it, they follow suit, and become excessively fond of it.

W. B. TEGETMEIER

Purple Verbenas

HAVING now read for the first time the letters in NATURE regarding the preference that sparrows show for the yellow crocus, it might perhaps help to elucidate the problem were it known that the choice of colour is not only confined to birds, as a few years ago our garden was infested by rabbits and there was a row of eight beds planted in turn, with white, red, and purple verbenas. The flowers of the red and white were eaten close off, whilst those of the purple were never touched. This happened three years running, since which, the garden, being protected by wire netting, has remained undamaged.

A. M. DARBY

Japanese Mirrors

YOUR correspondents, Messrs. Atkinson, Highley, and Darbishire, have referred to several conjectures and experiments respecting the curious Japanese mirrors and the patterns they reflect. None of these gentlemen have, however, referred to the suggestion offered by Sir David Brewster in the *Philosophical Magazine* for December, 1832. In this paper Sir David drew attention to some similar phenomena in the light reflected from the surfaces of burnished buttons of metal, arguing that in the mirrors (of which at that time he apparently had seen no actual specimen) there were slight actual inequalities of surface, artificially produced, but concealed from observation by their slightness of depth and by the brightness of the polish. This, of course, may be independent of the particular figures raised in relief on the back, as in the case cited by Mr. Darbishire; and so thought Sir David, for he added:—

"Like all other conjurers, the artist has contrived to make the observer deceive himself. The stamped figures on the back are used for this purpose. The spectrum in the luminous area *is not an image of the figures on the back.* The figures are a copy of the picture which the artist *has drawn on the face of the mirror*, and so concealed by polishing that it is invisible in ordinary lights, and can be brought out only in the sun's rays."

I trust Mr. Atkinson may be able to learn in Japan the real process of manufacture of these curious toys. Meanwhile are there not specimens in many of our museums that would repay examination? Were there not some amongst last year's exhibits at the Loan Collection of Scientific Apparatus?

SILVANUS P. THOMPSON

University College, Bristol, June 25

NOTE ON THE ELECTRICAL DISTURBANCE WHICH ACCOMPANIES THE EXCITATION OF THE STIGMA OF MIMULUS LUTEUS

MANY years ago my attention was drawn to the excito-contractility exhibited by the lipped stigma of *Mimulus luteus*, the structure of which I then gave an account of in the *Proceedings* of the Edinburgh Botanical Society. In connection with my recent investigation of the excitatory variation in *Dionæa* I have, during the last few weeks, in co-operation with Mr. Page, made experiments for the purpose of ascertaining whether in this organ, as in the leaf of *Dionæa*, the change of form provoked by mechanical stimulation is accompanied by a similar electrical disturbance.

Mimulus luteus is a favourite window plant on account of its showy flowers and the facility with which it can be cultivated. The mechanism of the contraction of the stigma can be best studied in the inferior of the two lobes, of similar size and form, of which the organ consists. In the unexcited state, when the flower is in full bloom, this lobe is curled outwards. The curling outwards is due, as I long ago observed, to the turgidity of the layer of loosely connected conducting cells, ending in papillæ, which constitute the stigmatic surface. So long as this tissue is turgid the elastic lamina by which it is backed is prevented from straightening itself, so that the whole lobe forms a scroll of which the axis is transverse. The effect of touching any part of the lobe, and particularly the papillary surface, is to diminish the turgidity of the tissue, as the result of which the organ slowly expands so as to face and ultimately meet its fellow.

The excitatory change of form which I have described is, as in the case of *Dionæa*, associated with an electrical disturbance of which the following are the most important features:—(1) The sign of the variation is the same as in *Dionæa*, the excited structure becomes negative to the rest of the plant. (2) The extent of variation is somewhat less than in *Dionæa*, the electromotive force developed between the stigma and style being usually about 25-thousandths of a Daniell, whereas in *Dionæa* the variation may amount to from 40- to 50-thousandths. (3) The variation is of relatively long duration; it reaches its maximum at the ordinary temperature of summer, about five seconds after excitation. It subsides at first rapidly, then very gradually, so that the effect may not have entirely passed off until two or three minutes have elapsed.

As in *Dionæa*, the period of electrical disturbance is shortened by increase of temperature. Thus in five stigmas in which the period was measured at 20° C. (68° Fahr.) and at 37° C. (98° Fahr.), the mean duration of the interval of time between the commencement of the electrical disturbance and the moment at which it began to subside was 6·2 sec. at the higher temperature, and 3 sec. at the lower.